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Biogeosciences Discussions

Interactive comment on "Distinct patterns in the diurnal and seasonal variability in four components of soil respiration in a temperate forest under free-air CO_2 enrichment" *by* L. Taneva and M. A. Gonzalez-Meler

Anonymous Referee #2

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In the manuscript titled "Distinct patterns in the diurnal and seasonal variability in four components of soil respiration in a temperate forest under free-air CO2 enrichment", the authors: L Taneva and MA Gonzalez-Meler use component-exclusion method to separate total soil respiration at their study site into heterotrophic respiration from the mineral soil and heterotrophic respiration from the litter layer and, by difference, also into autotrophic respiration. They further subdivide the signal from heterotrophic mineral soil respiration into two age classes: old (prior to CO2 enrichment) and young (post-CO2 enrichment) with the use of C-13 isotopes. The study is carried out for two

growing seasons (June to October) in 2003 and 2004, with monthly measurements. While the experimental results can be of much interest to BGC audience, I think the ms should undergo major revisions, including potentially some additional analysis, before being accepted for publication.

More detailed comments: 1. While the authors present differences in the day and night patterns and growing season trends in the different Rs components they studied, I think the ms can be improved by also including an analysis into any attempts to try to explain the observed patterns. The authors do mention some analysis with respect to soil temperature and moisture effects, but do not go into details. Furthermore, the lack of correlations between Rs components and for example soil temperature and moisture to which they refer, may be due to the fact that they use 10 cm deep soil temperature and moisture measurements only. One would expect different components to perhaps respond to different driving variables, as mentioned in discussion and introduction. For example, respiration from the litter layer, one would expect to be correlated more with air temperature as compared to mineral soil temperature at a 10 cm depth, likewise precipitation events may be more important compared to soil volumetric water content in this case. Have the authors considered these aspects in their analysis?

2. The statistical analysis used to analyze the data set included the application of mixed effects models. The authors should elaborate more on the results. In the current ms they seem to only present %-differences and some p-values. I think the readers would benefit more from a more detailed explanation of the types of models they fitted to their data and the results of these fits – which of the variables turned out to be statistically significant in explaining their trends. Were temporal driving factors mentioned on line 24 (P 2.9) tested together or separately, which ones were important? Was data controlled for temporal autocorrelation and were temperature and moisture considered in the analysis, especially in explaining seasonal trends? Which brings me to their observation that Rs in 2003 was higher compared to 2004, yet no explanation of this result is provided or discussed (P 3.1). Seasonal /interannual variability is not discussed in

detail.

3. Be more clear in what you measured and presented, Rs components : Rr, RL, Rsom is one part of your experiment; separating Rsom into old and young part is another part. Right now it all seems mixed up together and unclear in Abstract, Intro and Results (ex. 3.3.1 goes better with 3.4 and 3.5)

4. Not sure about journal policies, but I doubt it is allowed to reproduce word for word what has already been published previously without reference. Your sections 2.1 and 2.2 read exactly as in your 2008 publication in Soil Biology and Biochemistry.

5. Figures/Tables: Table 2 and Figure 1 and 2 seem repetitive; Table 3 – which year of data is presented 2003? 2004?or mean of both? – figure 3 seems like a repeat of Table 3; Figure 1 could be better interpreted if also include soil moisture and temperature variability at the time. Figure 4 – show legend for black/white bars. Suggestion = Might be better interpreted if you normalized your emissions for each case and then compared normalized values. Otherwise, explain the temporal trend observed (i.e. decrease over the growing season in post treatment, but increase in pre treatment).

6. Technical/editorial comments: the writing can be improved.

a. Some of the sentences are too long (ex. L 8-12,Abstract; lns 3-10 and 15-18, Introduction; ln.2-5, page 2880 is too long + the sentence after is not clear; ln. 15-20, pg. 2893)

b. others are confusing/unclear: ex. Paragraph 3.1. line 11 = should "-5%" read +?; ln15-17, pg 2879 unclear, remove one of the "both"; ln.5-9, pg. 2877 unclear, rephrase; ln 9-10 Abstract – disagree – I think there was more emphasis on biotic controls of Rs as opposed to abiotic lately.; ln.13-14, pg.2878 unclear; ln.5-9, pg. 2880 unclear; ln.5-6, pg. 2884 unclear; ln. 21pg.2888 "significantly"; ln. 25 pg.2888-2889 no clear; ln.21, pg. 2889 – results refer to enriched or ambient? ; paragraph 3.5 too long and repetitive; ln. 8-10 check the works by R. Vargas, M. Carbone, D. Gaumont-

Guay, P.Jassal, G.Saiz; ln.18, pg. 2893 "in situ" refers to which measurements? My understanding separation was done based on lab incubations of separated sieved soil samples – that is not "in situ". Please clarify; ln.26 pg. 2893 to ln.2, pg.2894, not clear; ln.5-6 pg.2894 "presence" or "absence" not clear; process-based Q10 approach? Not clear – ln.12 p.2894.; ln. 8-10 – not clearly shown in the ms, as stated.; ln.10-13. Pg. 2895 – reference?; ln.26-27, pg. 2896 unclear; ln.1-4, could skip.; sections 3.1. and 3.2 seem to be repetitive; ln. 13-18, pg.2894, from what I recall, in Subke et al 2006 publication, studies that separated Rs components using C-13 methods, were not consistent with the other methods – you used C-13 here, so how does your study fit with the rest?

7. Interesting points to keep and elaborate on: 21-22, pg. 2895; ln.20-23, pg.2896; ln.10-14 pg.2897.

8. References – did not check.

Thank you.

Interactive comment on Biogeosciences Discuss., 8, 2875, 2011.